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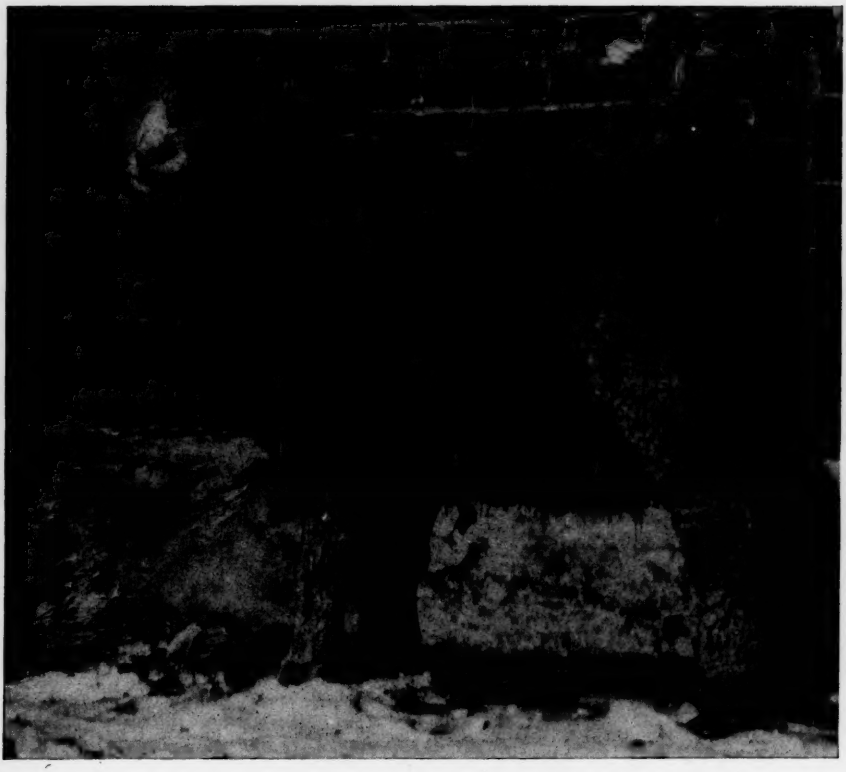

DILEMMA COMPLETE WITH HORNS

(SEE PAGE 149)

Overcrowded Puerto Rico

Fuzzy Cotton

Taste-Blindness of Diabetics

Tetraploid Tomatoes

Russian Genetics

VOLUME 38 MAY, 1947 NUMBER 5

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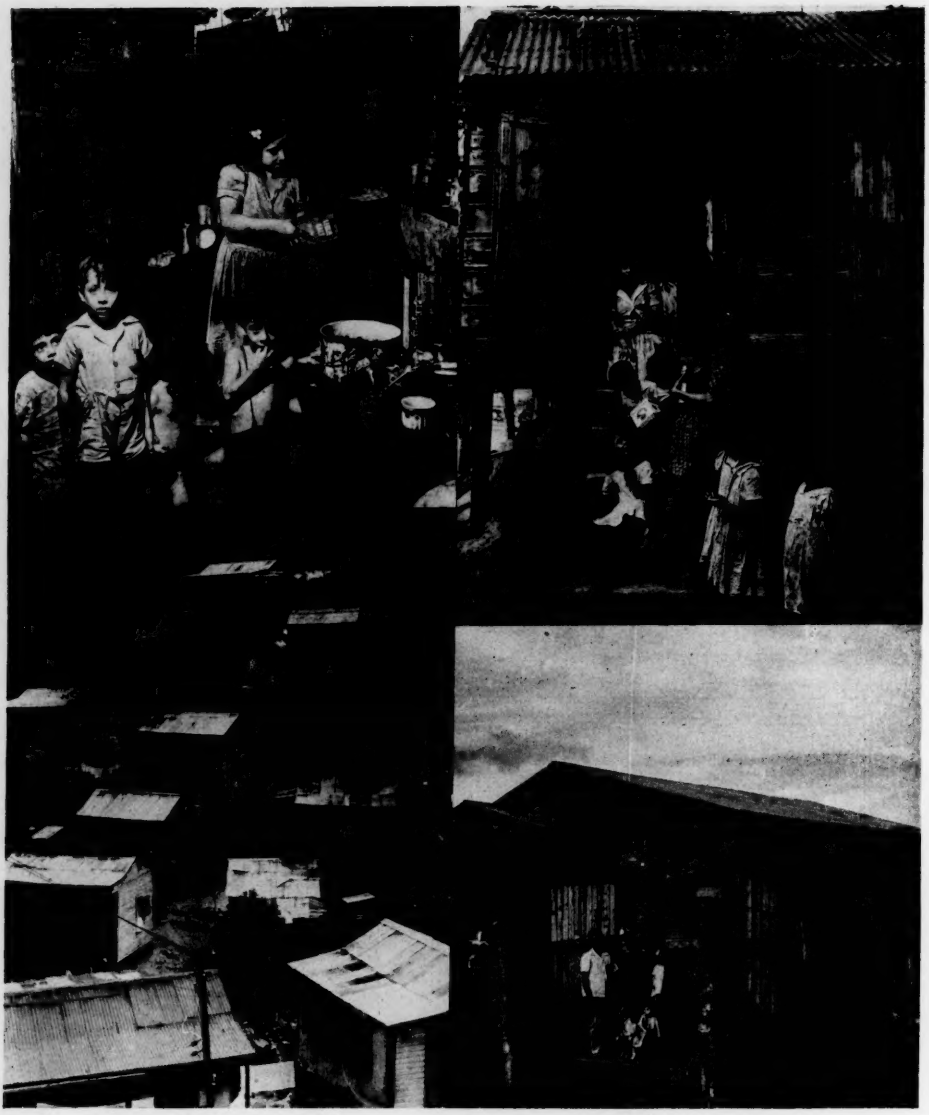

THE PHYSIOGNOMY OF A STATISTIC

Figure 1

In Puerto Rico in 1940, the rate of natural increase was 21 per thousand. Beneath the staid and sober language of demography lies the misery of people: of the little girl who is not playing at keeping house for the entire family while her parents are away working; of people who live out their days, intimate with hunger and ignorant of "privacy." In Puerto Rico, as everywhere in the world, overpopulation means people, too many of them with too little space to give play to the human spirit. (Photographs by Rosskam and Delano, courtesy of Office of Information for Puerto Rico.)

POPULATION PRESSURES AND THE FUTURE OF PUERTO RICO*

CLARENCE SENIOR
Secretary, Asociación de Estudios Poblacionales de Puerto Rico

THE basic facts on population in Puerto Rico are fairly widely known. The population has doubled in the past 50 years. The density has risen to over 600 per square mile, approximately 15 times that of the United States. The rate of natural increase in 1940 was second only to that of the Soviet Union in the period immediately before the war. The Russian rate is 23.2; the Puerto Rican 21. These rates might well be compared with the following: United States, 6.1; Germany, 7.5; Italy, 9.3; India, 11.5; and Japan, 12.5. Countries which come closer to the Puerto Rican figures are Egypt with 16.0; Jamaica, 15.7; and the Philippines, 15.4.

Various factors, including the age distribution of the population, indicate that Puerto Rico's birth rate will continue at a high level unless the people become sufficiently aware of the situation consciously to modify this trend. The cumulative effect of this high rate of natural increase can be summed up in the figures of the net average annual increase in the population of the island from 1930-40 which was 32,534 as compared with the projected annual increase of 42,551 for the decade 1940-50.

Is Puerto Rico Overpopulated?

There is an inclination in certain circles to deny that Puerto Rico is overpopulated. Let us examine that contention. The statement is sometimes made that "Puerto Rico does not have too many people; it has too few resources." To the unemployed city laborer or the landless farm worker this statement must appear as a cruel jest. Natural resources and people are as inseparable as two sides of a coin. The ability of a given area to support people is determined by the interaction of three factors: (1) the number of people; (2) the natural resources; and (3) what the people do with those resources. A sparsely settled territory, even though richly endowed with natural resources, will not furnish a high level of living for the population unless the people take advantage of the resources. On the other hand, a densely settled area with few resources has to struggle to the utmost to wrest a living from the area.

So far as land is concerned, Puerto Rico now averages less than one acre per capita as compared with 2.2 in 1899, 4.6 in 1846, and 45.6 in 1765. *If arable land alone is considered, each person now averages somewhat less than half an acre.* The amount of rich land in the coastal area is drastically limited and the small acreage of uneroded mountainland offers few possibilities for significant increases in production. There is no question in the mind of any person who has studied the geology and geography of Puerto Rico that the island is extremely limited in natural resources. Metals and minerals on which present-day large-scale industrialism is based are almost entirely lacking.

Puerto Rico has the disadvantage of coming late onto the scene of modern international commerce. Thus far its chief products have been protected by the United States tariff. The present drive of the United States for the lowering of trade barriers presents additional obstacles to the enrichment of the Puerto Rican economy. Puerto Rico must do some things that are already being done in the world but do them much better than its competitors and it must find something entirely new and "get in on the ground floor." Obviously every new possibility in plastics, chemurgy, electronics and atomic power must

*From a speech delivered to the Church Rural Life Conference on February 20, 1947.

be investigated and utilized to the utmost. However, it seems to me that only an irresponsible optimist would base public policy on any great expansion in the economy. The population will probably reach 3,000,000 by 1960 unless drastic changes take place soon. There is little indication that the island can sustain such a number without a disastrous drop in levels of living.

Possible Solutions

These circumstances of a small land area, scarce resources and ever-mounting population must be kept in mind in assaying public policies. There seem to be three approaches to these problems. The first and possibly the most widespread seems to be that of drift. This approach results not only from human inertia but from a deliberate program advocated by large and powerful groups who claim to speak in the name of God. They argue that "God sends children and nothing should be done to interfere with the will of God." It seems to me that this stand must be challenged. Presumably, God gave man both sexual organs and intelligence. The latter should be used at least as frequently as the former. Rabbits, with no intelligence, seem to be the models held up by some to be followed by good Christians.

Drift, which means that intelligence will continue to be applied to the prevention of deaths but not to limitation of births, will result in continued serious pressure on resources. It means either continued dependence on funds from the United States or a collapse of the insular economy under the weight of population.

The second approach, which is characteristic of many government officials and intellectuals, is what I dub "economic particularism." It is summarized in the frequently heard statement "raise levels of living and the population problem will solve itself." This statement needs examination.

Undoubtedly raising levels of living will help but it is fallacious to assume that nothing can be done until levels of living are raised. It is true that on the whole people with higher incomes have fewer children. It is also true that people with higher incomes are better educated. Why must it be assumed that educated poor people would not see the wisdom of limiting family size? There is every indication that poor people in Puerto Rico would welcome information on the prevention of unwanted children just as eagerly as rich people do. To deny the poor people the right to have children only when they want them is as undemocratic as to deny them the right to any other type of education or participation in the good things of life made possible by modern wealth production.

Moreover, a desire to maintain living standards is not the only reason for conscious birth control. Throughout history there have been cultures in which people limited birth for various reasons besides the economic. Lysistrata, it will be remembered, called upon the women of Greece to refuse to bear children until the men stop butchering them in continuous warfare. Numerous conquered tribes in Africa and the Americas have deliberately reduced their births as part of their resistance to conquest. Anthropologists, in a recent comparative survey of human reproductive practices, found contraceptives being used in 16 out of 25 contemporary primitive tribes.

On the other hand, the Indians of the United States, who have known increasing prosperity for the past 20 or 30 years, now display the highest rate of natural increase of any group in the United States.

Let us continue our examination of the "level-of-living" argument. Data gathered in June, July, and August of 1946 by field workers under the direction of Dr. Lydia J. Roberts show that a drop in family size of rural households does not take place until an income of between $1500 and $1999 is reached. Urban households show a perceptible drop in number of children at between $2000 and $2499. Consider, now, that the average family incomes for 1946

were as follows: wage earners $596; rural families, $633; all families, $829; urban families $1,092.

We also know that family incomes (for wage earners) increased between 1941 and 1946 at the rate of $40.83 per year (at the 1946 price-level). If we choose $1700 as the point at which a drop in family size will take place and if we further assume that the insular income will continue to increase at the rate of the previous five years *it would take 27 years* for the average wage earner family to be raised to the income class in which a significant drop in the family size would be recorded. It would be nothing short of a miracle if the 1941-1946 income increases were to be continued. In fact, all economic projections indicate that a drop, rather than a rise in per-capita income is to be expected within the next decade or so.

Obviously there is something fundamentally wrong with this argument. For reasons of logic, sound economics and democratic principles, this approach must be ruled out. A third solution holds that education for voluntary parenthood must be taken to the poorest sectors of the population.

Wasted Births

There is another factor which should be taken into account in stressing the necessity for increased education for the poor. All of our evidence shows a terrifically high mortality for both mothers and children which is directly related to the large number of births. The infant mortality rate in Puerto Rico in 1943 was 95.3 per 1000 live births as compared with 40.4 in that same year in the United States. The United States itself has an indefensibly high infant mortality rate, much of which is accounted for by the large, deeply ignorant sectors of the population of the southern states. Maternal death rates in 1942 were 41.7 per 10,000 births in Puerto Rico as compared with 5.4 in the United States. Recent investigations conducted by Dr. Lydia J. Roberts have shown that among large families as many as 43 per cent of the children born have died. Similar figures could be given for stillbirths and abortions. All of these figures add up to a distressingly high wastage of human life. Surely we cannot rest until we have adequately tackled this aspect of involuntary parenthood. The psychological aspect of the production of unwanted children is a facet of the problem which I will mention only in passing.

Former Governor Tugwell, in his message to the 16th Legislature, speaks of "the failure of Puerto Rico's birth control law to affect birth rates in the slightest degree." The fact is, however, that the application of the law has largely been entrusted to its enemies and it has been deliberately sabotaged. It must be realized that no law applies itself automatically and that a law subject to as much opposition and misinterpretation as the birth control law needs special efforts to be put into practice effectively.

A Balanced Population Policy

There is a growing belief that neither the policy of drifting nor the policy of concentrating almost exclusively on income improvement is defensible under the present extremely serious conditions. A growing group is trying to work out a balanced population policy, taking economic and social factors into account. It believes, of course, in the greatest possible expansion of industry and any other means of raising the productivity of the Puerto Rican people. It believes, however, that we cannot depend exclusively on economic improvement, which at best is going to be achieved only with the greatest difficulty and over a considerable period. Consequently, it maintains that every possible means must be taken to call to the attention of the Puerto Rican people the two main factors: (1) that resources and wealth are limited and can be expanded only slowly and (2) that under present circumstances the more mouths there are to be fed, the less food there will be for each mouth. It is the question of *relative* growth of wealth and population that is crucial.

Much can be done through education, particularly if it is based upon the growth of the individual and the strengthening and improving of the democratic family life. Education must also have as one of its foundation stones the giving to each man and woman a role to play in the community, economically, politically and socially. Education can cut down the family size both by raising the age of marriage and by showing people how to plan their families.

Demand for Contraceptive Knowledge

Education in the use of contraceptives would be an extremely difficult job if there were no demands for this information. However, this is not the case. There is an abundant evidence that there is a widespread, persistent demand for knowledge of how to avoid unwanted children. A study of the *municipio* of Lajas conducted by Morris Siegel and Marguerite N. King showed an almost unanimous demand for contraceptive knowledge among the women interviewed. It showed only a few persons using modern contraceptive methods but many using crude herbs, drugs and even magic to avoid conception. Similar studies made by Drs. Belaval and Beebe in 1941 and by the writer among the Puerto Ricans in St. Croix in 1946 and the experience of birth control clinics and hospitals furnish a mass of evidence for the existence of such a demand. The wide spread increase in sterilization in the past few years is another weighty bit of evidence. This operation, formerly confined to rich women, is now in demand at both public and private hospitals in spite of the opposition of certain groups and of the drastic nature of the step.

Emigration has a minor role to play in any balanced population policy. It is an expensive device which sends people away from the island after they have been raised and trained at the expense of the economy, which afterwards receives little or no compensating benefits except the absence of a mouth to be fed. Emigration from Puerto Rico in all of the years since 1908, when data are first available, has resulted in a comparatively insignificant net out-movement of only 100,000. Emigration must be linked with many other methods in attacking the problem of over-population. The cost of emigration must be considered, and balanced against the costs of other means of attacking the problem.

Thus, what we must work toward is a well-rounded program, attacking on all fronts; industrialization and other means of increasing productivity, greater food production on idle land, general education, raising the age of marriage, strengthening family ties, emigration, and the spreading of knowledge of how childbearing can become a voluntary matter among the poor as it now is among the rich.

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THE ASSOCIATION OF DIABETES AND TASTE-BLINDNESS

M. C. Terry, M.D., and Gabriel Segall, M.D.*

SEVERAL years ago, one of us (M.C.T.) became interested in the phenomenon known as taste-blindness through articles published in the Journal of Heredity.[3,6] Taste-blindness is defined as the inability to taste the compound phenylthiocarbamide (PTC) when presented either as "the pure dry crystals or a five per cent PTC solution."[3] The tests are commonly made by having the subject chew a small piece of paper that has been impregnated with a solution of PTC and then dried. Initially a short series of tests was run primarily as a demonstration to an interested group of doctors and nurses at the U. S. Veterans Hospital at Knoxville, Iowa. The results, in general, were in accord with those obtained by other investigators. It happened, however, that there were several persons with diabetes mellitus among those tested, and these showed an unusually high frequency of non-tasters. Recently an opportunity arose to test whether this apparent association could be confirmed in a larger survey. Since the incidence of diabetes mellitus is said to be relatively high among Jews, we thought it well to collect data separately for Jews and non-Jews, and also for Negroes, because it is said, rather doubtfully, that diabetes is rare among them.

It has been established by geneticists that the inability to taste PTC (when presented in the usual manner) is inherited as a Mendelian recessive.[1] The frequency of persons unable to taste PTC has been found to be about 25-30 per cent in the general population, while the remaining 70 to 75 per cent experience a definite bitter taste with the compound. Also it has long been known that the incidence of diabetes mellitus in the families of diabetic patients is relatively high. The hereditary component in this instance is generally recognized—see, for example Cecil[2] and Wilder.[7] Pincus and White[5] have suggested that diabetes mellitus is a recessive trait with variable expression.

The Population Tested

We have tested† 680 persons, distributed as follows: 262 Jews, 250 non-Jews, and 168 Negroes. Of the 262 persons, listed as Jews, three of one family may not be Jews, although they have a Jewish name, and four are half-Jews. These seven cases, fairly balanced as to diabetic and non-diabetic tasting and non-tasting, could be removed from the Jewish list without material effect; but this has not been done. The 124 Jewish diabetics are verified by hospital records and the 138 Jewish non-diabetics are almost equally well authenticated. The non-Jewish diabetics are similarly veri-

*U. S. Veterans Administration, Retired, 1940 Waverly Street, Palto Alto, Calif., and Associate Senior in Medicine, Cedars of Lebanon Hospital, Los Angeles, Calif., respectively.

†The test paper used in this series of observations was supplied to us by Dr. G. W. Beadle of the Division of Biology, California Institute of Technology. The paper was prepared by soaking filter paper in a one per cent solution of PTC in acetone and allowing it to dry in air.

We are indebted to Dr. Beadle not only for the paper, but for suggestions as to possible genetic interpretations. We are also indebted to the members of the staffs of the Stanford University Hospital, the diabetic clinic of the University of California Hospital, and the Cedars of Lebanon Hospital, for the opportunity to test both diabetic and non-diabetic patients; to Dr. S. Z. Goodman, Chief of the Diabetic Department and to Dr. Roy Popkin, head of the Peripheral Vascular Clinic of the Cedars of Lebanon Hospital for their cooperation; to many private practitioners of medicine, including particularly Negro doctors and to various laymen, both Negro and white. Dr. Willis H. Rich of the School of Biological Sciences, Stanford University, contributed to the statistical analysis and the preparation of this report.

fied either by hospital records or by the word of physicians known to us. We cannot say as much for the non-diabetic groups of non-Jews and of Negroes, because they include persons on whom clinical data are not available. The members of these latter groups were adult, but many had not reached the decades of maximum diabetic incidence. The number of known diabetics among Negroes is so small that we merely record our findings without further comment. The majority of the diabetic patients are under treatment with some form of insulin and diet. In many of the non-diabetic controls, frequent urine analyses were made. In order to avoid suggestion as to taste, patients were tested in separate rooms whenever possible. In cases where this was not possible, they were asked not to disclose their taste-reactions to other patients. In case of doubt as to taste reaction of known diabetics, individuals were tabulated as tasters. Table I presents the data for each ultimate category.

The statistics given in the last column of the table are the basis for our application of the standard test of significance of observed differences in the percentage of taste-blindness in each group (see Pearl[4], Appendix X, p. 517).

Group Comparisons

Our data permit of five comparisons that are reasonable and pertinent to the inquiry. These are: 1) Between Jews and non-Jews, non-diabetic. The difference is five per cent and P is greater than .3. The difference is not statistically significant. 2) Between Jews and Negroes, non-diabetic. The difference of 2.9 per cent is not significant. 3) Between non-Jews and Negroes, non-diabetic. The difference is 7.8 per cent and P is almost exactly .1. The difference is not statistically significant by the usually accepted standard, but suggests the possibility of a real difference. 4) Between Jews and non-Jews, diabetic. The difference of 7.5 per cent is not significant; P is greater than .2. The number of Negro diabetics is so small that comparisons with the other racial groups of diabetics is not warranted. On the basis of these comparisons we have felt justified in combining Jews and non-Jews for the final comparison, 5) between diabetics and non-diabetics. The difference between these two groups is 15.3 per cent and P is less than .001. The difference is highly significant. This abundantly confirms the original observation that there is an association between diabetes mellitus and taste-blindness. This study does not attempt to pass upon the nature of this association. The determination of taste-blindness lacks much of the exactness of such procedures as blood-grouping, as anyone knows who has attempted it. It is not a purely objective test. In spite of this the observed association seems to be well established.

TABLE I. Association of Taste-Blindness with Race and with Diabetes.

Group	Total Number Tested	Number Taste-blind	Percentage Taste-blind	Standard Error of Percentages
Jews, diabetic	124	55	44.3	4.5
Jews, non-diabetic	138	32	23.2	3.6
Non-Jews, diabetic	87	32	36.8	5.2
Non-Jews, non-diabetic	163	46	28.2	3.5
Negroes, diabetic	11	0	00.0	
Negroes, non diabetic	157	32	20.4	3.2
Whites, diabetic*	211	87	41.2	3.4
Whites, non-diabetic	301	78	25.9	2.5

*White includes Jews and non-Jews—the sum of the two groups.

In a population in which the various genetic types exist in equilibrium frequencies, one would not expect the observed distribution to result from genetic linkage. It is possible that some diabetics may not taste phenylthiocarbamide (PTC) because of some physiological reason.

Two ways in which such an hypothesis could be tested have occurred to us. First, it would be expected that the frequency of inability to taste among diabetics would increase with time after the onset of the disease. Secondly, some non-tasting diabetics with non-tasting spouses would be expected to have tasters among their children. In other words, some of the phenotypically non-tasting diabetics would be genotypic

tasters. It is hoped that these possibilities may be considered further in a subsequent publication.

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◆

Genetic Effects of the Atom Bomb

* * * Japanese studies were initiated in Hiroshima on rabbits of the animal colony of the University of Literature and Sciences, about one mile from the hypocenter. Fourteen out of the thirty-two survived and were to be used in breeding experiments, but as a result of poor caging facilities and depredators (possibly human) the colony became depleted. Even fruit fly studies had to be given up for lack of facilities.

* * *

One of the welcome surprises to which we were treated was the attitude of the Japanese scientists. We had held the impression that they would need encouragement to work on the after-effects of the bombs. We soon learned that this would be unnecessary; they already had written 120 papers on various medical aspects of the atomic bomb and (as scientists would be) were more concerned with the possibility of publishing their work than with alleviation of the circumstances under which they had to work.

* * *

They are painstaking and objective; their objectivity found its reward in the enthusiasm with which the bomb effects were studied by men whose colleagues and families had been killed in the bombing. One man had even turned the site of his ruined home into a garden plot for the experimental study of bomb effects upon plant growth.

* * *

The scientific observations collected by our commission have been given in a report issued by the National Research Council and released by the War Department. The greater part of the report deals with the medical studies made by the Japanese, and was written by Dr. Tsuzuki. The acute effects of the bomb have been adequately described in earlier releases. These have been termed "atomic bomb injury" which is a syndrome resulting from the combination of thermal, mechanical, and radiation energy. Under the disorganized conditions resulting from the atomic bomb explosion, it is obviously difficult to separate these effects. Out of the picture, however, emerges the pattern of radiation damage in many of the victims, including loss of hair, extreme depression of white blood cells, and an increased bleeding tendency leading to blood loss under the skin and into body cavities.

The survivors of this acute radiation damage later show anemia and sterility. The latter was studied in a group of males, and it was found that recovery occurs. This acute radiation picture serves to verify what is well appreciated from experimental work on the nature of radiation damage.

One observation of the Japanese, that the mortality rate near the bomb center was higher in males than in females, is of interest, but lacks an explanation at the moment.

As of the time of our visit, the acute effects had apparently passed, and there was little to be seen. The only striking and consistent finding was the state of burn effects. Many of the burns healed with accumulations of large amounts of elevated scar tissue, or keloids. These scars continue to grow and become more elevated for eight to ten months, at which time they reach maximum growth.

Keloids present a problem of great interest. They are by no means unknown following various types of injury which heal with scar formation, and it has been noted that they are a much commoner response in negroes than in the white population of this country. There are undoubtedly other factors involved in susceptibility. It was found that keloids were also common in Japanese patients who had been burned by flash explosions, as from gasoline vapor, and it is entirely probable that the thermal injury of the bomb is responsible for the keloids rather than exposure to radiation, since they have occurred at considerable distances from the bomb center.

The work of studying after effects of atomic casualties will be a considerable one for various reasons. Among these is the fact that public information on this subject is likely to create undue excitement. For example, an unofficial story which gained newspaper publicity soon after the first bomb stated that nothing would be able to live in the bombed area for 70 years. This story gained much more recognition in Japan where it was received by radio than any of the later accurate information, and when our party was there 1½ years later, it was still creating considerable concern, especially in Nagasaki.

The question of genetic effects of the atomic bomb is one which is likely to become involved in difficulties of this sort, especially in view of the vivid statements by Professor Haldane regarding the possible deterioration of the human race from the emergence of recessive genetic effects. There is certainly no evidence at the present time indicating that abnormal infants have been produced as a result of bomb radiation, yet the lay or pseudo-scientific mind is likely to seize upon a single report of such a birth in a bomb victim (such as occurs in any location, such as Chicago, with known frequency) as evidence of an atomic bomb effect.

A definite attempt is being made to determine the results of all conceptions in the Hiroshima area, organized by the Japanese National Research Council. This is a part of a larger enterprise, in which the Hiroshima prefecture has collected histories from no less than 160,000 persons surviving the blast, with data on distance from the hypocenter, position, symptoms (if any) of radiation sickness, and so on.

It is, of course, necessary to set up a control area in which the normal pattern of health can be studied in a similar population, since any changes resulting from the bomb may be expected to be small and could probably not be detected at all without reference to a Japanese population existing under comparable conditions of nutrition and other attritional effects of the war. The city of Kure, 25 miles from Hiroshima, has been tentatively selected for this purpose. Initial steps were taken by holding meetings with the leading physicians and midwives of Kure, presenting them with this possibility. It will require little thought to appreciate the magnitude of such a program.—AUSTIN M. BRUES, *Bulletin of The Atomic Scientists*. June, 1947.

HUMAN GENETICS

ADVANCES in the study of human genetics during the past two decades, and an increasing interest in that field on the part of the medical profession require that the new literature be critically reviewed and our ideas about the genetics of human characteristics be re-evaluated. The author attempts to present the vast literature on this subject in *Human Genetics*.* The two volumes were prepared as a sourcebook for those concerned with the genetic aspects of medicine and anthropology. This is a meritorious aim, but the reports concerning human characteristics—normal and abnormal—are so abundant and so varied in quality that it is a most difficult undertaking.

As a whole the work is encyclopedic. The number of traits referred to is so great that space cannot be used to enumerate them. There are 42 tables and 326 figures, including 160 representative pedigrees and 126 well-chosen photographs showing human characteristics. The use of photographs in a book on human genetics is to be recommended since they often present a trait much more accurately than is possible by a verbal description. Lists of the pertinent literature fill approximately 15 per cent of the pages. A few mistakes have occurred, as is almost inevitable in any long list of literature. The author places his bibliographies at the end of each chapter, including in them the literature of the specific topic under discussion but not repeating a reference given in another chapter. On occasion this may cause the reader considerable loss of time and temper since page references to earlier lists are not always given.

In the first four chapters the author presents a relatively brief but concise discussion of the general principles of heredity and of normal and abnormal cytology of man. A list of genes inherited with the *X*- and *Y*-chromosomes is given. The discussion of the phenomenon of linkage in man includes a list of autosomal traits for which the evidence —any evidence—indicates probable or possible linkage. A map of the sex-chromosomes shows the relative positions of some of the genes on these chromosomes. The reader is introduced to the idea that man is a tetraploid.

Individual and racial differences are discussed in the remainder of the work. The characteristics are separated under proper chapter headings into related groups. They include not only pathologic conditions, but traits such as eye and hair color, shape of scapula, and levels of differentiation of the vertebrae. A chapter on the inheritance of nonabnormal mental differences is a welcome addition to a subject which so abounds with the abnormal. Unfortunately, the methods used in presenting this material are as faulty as those referred to in other parts of the work. In one chapter, the author discusses the theories of causes of cancer (somatic cell mutation, virus action, and effect of carcinogenic substances). He reports the literature on histories of certain forms of cancer, such as retinoblastoma, which are recognized to have a hereditary basis, and briefly summarizes the literature on mammary cancer and that on the inheritance of cancer in mice. Little attention is given to genetic studies of human breast cancer or of the effects of extrinsic factors upon mammary carcinoma. The author concludes this chapter with a summary, a method he unfortunately failed to use elsewhere.

Balanced Probabilities

One who attempts to use the work will have mixed feelings of pleasure and irritation, with the latter probably predominating. He will pick up some interesting bits of information about certain traits. He will read some passages

*R. RUGGLES GATES. *Human Genetics*. Pp. xi + 1518. Two volume set $15.00. The Macmillan Co., New York. 1946.

which will be so confusing as to detract greatly from the value of the work. In his attempt to explain the higher frequency of hereditary clubfoot in males than in females, the author expresses the peculiar idea (p. 391) that "possibly the *Y*-sperm bearing the gene for clubfoot on an autosome is more viable than the *X*-sperm bearing this factor." The author cites a history of tylosis in which an affected parent was reported (by a descendant some generations later) to have had 11 children all of whom were affected and the suggestion is made that a homozygous mutation to dominant tylosis had occurred in that parent. In a history of hexadactyly the author offers a like explanation to account for 12 polydactylous children, although in a later reference to this history he does suggest another possible explanation. It is more logical to assume not that the parent in each case was a homozygous mutant, but that a heterozygous person by chance produced only one type of progeny or that pedigrees based on family tradition are not very accurate.

In a reference work prepared for persons who have had little training in genetics, it is essential that the material be presented clearly and concisely and that accurate descriptions of human characteristics be given. Interpretations about the methods of inheritance should be as exact as it is possible to make them after the pertinent literature about a hereditary trait has been critically reviewed. New ideas and hypotheses lacking adequate support should be limited to a bare minimum in order not to befuddle the reader. In many instances the author has failed to meet these standards.

Although the author frequently gives a vast number of references to the literature for any specific trait, he does not present the data in a manner that is helpful to the general reader. Usually the author gives a brief summary of each report and the conclusion of that investigator. Sometimes the conclusions of two investigators are diametrically opposed to each other. A reader is consequently placed at a disadvantage because he does not have the two reports before him to draw his own conclusions, and the author frequently fails to suggest where he suspects the truth to lie. Hence the reader will often know little more about the method of inheritance of a trait than before he read the book.

Chromosomal High Jinks

A suggestion which occurs entirely too frequently throughout the text is that chromosomal aberrations or abnormal chromosomal inheritance may account for the lack of a clear-cut manner of inheritance of a trait in some pedigree. "Clear-cut manner" in this sense means a text-book rule of inheritance. Although continued repetition will not increase the truth of a statement, its efficacy in persuasion has long been established. One can only hope that a reader untrained in genetics will not be misled or overly confused by the author's yen for improbable postulation. Several family histories are cited wherein a trait has passed from mother to daughter to granddaughter. This the author assumes is to be accounted for by an end to end attachment of the two *X*-chromosomes in the female, thereby causing them to pass together from female to female. The phenomenon is well known in Drosophila and hologynic inheritance has been used as a probable explanation of a history of color-blindness in man. The author suggests that in some other family histories for traits, including cataract, epiloia, early dental decay (published in 1863), and missing upper lateral incisors, this hologynic inheritance has occurred. The random deviations from expectation surely are not great enough to justify a highly speculative alternative explanation.

Crossing-over between the *X*- and *Y*-chromosomes (presumably the non-homologous parts) is referred to by the author on several occasions. According to the interpretation, the dominance of a gene is changed when it is transferred from one chromosome to the other. The

fact that the non-homologous parts of the chromosomes do not carry the same genes does not deter the author, and he offers no explanation for this rather fanciful phenomenon.

In a family history of ichthyosis, for example, the trait was usually transmitted through normal females to some of their sons as a regular sex-linked recessive, but one affected male had an affected son. The author suggests that the gene on the *X*-chromosome crossed over to the *Y*-chromosome to cause that one exception. It would also be necessary to account for the dominance of the gene on the *Y* over the normal allele on the *X*. In a history of bleeding, considered by the author to be hemophilia, the trait passed from fathers to sons in part of the history and through normal females to their sons in another part. The suggestion is made that here crossing over removed the gene from the *Y*-chromosome to the *X*-chromosome where, on the basis of the history, it then behaved as a recessive. A history of syndactyly is shown (Fig. 163) in which the trait occurred more frequently in males than in females. One affected male produced only affected children, four males and three females. The author is unwilling to assume a simple explanation for the inheritance, but states instead that (p. 436) "If the condition were an autosomal dominant, this would be unlikely, since he is presumably heterozygous, but if the gene were in the *Y* with crossing-over to the *X* in some of his germ cells this might well happen." Although an individual heterozygous for a dominant trait can be expected to produce two types of children, he may just as a matter of chance produce seven children all alike for the trait. Families of seven boys or seven girls are not unknown, and this example is statistically comparable. There is no reason to assume a less probable phenomenon to account for the transmission of the trait. Very often the explanations Dr. Gates proposes seem inherently far less possible than the "conventional" explanation of random deviation. A failure to consider the statistical aspects of human genetics is an unfortunate feature of this book.

Multiple Mutations

The author is intrigued by the fact that a disease condition such as nystagmus is inherited in some families as a dominant, in others as a recessive, and in still others as a sex-linked recessive. He makes the assumption that one specific gene is involved and that the different methods of inheritance result from changes in position of the gene on the chromosomes (translocation), with modifiers also affecting the dominance. This interpretation is applied not only to different family histories having like anomalies, but sometimes to a pedigree of a single family in which the trait can be interpreted as a dominant trait in one part and, as sex-linked in another part of the pedigree.

This phenomenon, "peculiar to man," is reported for many disease conditions. The climax is reached in the chapter on hemophilia where the author spends considerable effort to discredit the generally accepted criteria for hemophilia. The author makes several conclusions regarding hemophilia which are contrary to opinions held by many others who have investigated the disease. He states that: (1) the existence of hemophilic females is proven by abundant facts; (2) it is highly probable that homozygous hemophilic females exist; (3) hemophilia while generally sex-linked, is dominant in occasional families and probably recessive in rare cases; and (4) that possibly prolonged clotting time has been overemphasized as an essential diagnostic feature of hemophilia. In support of his assumptions about hemophilia, the author refers to some female carriers who bled easily and cites some family histories of hemorrhagic diseases which, in contradiction to the opinions of the original investigators, he considers to be hemophilia. In other cases he refers to bleeder women who were reported to be hemophilic. If we agree that hemophilia was correctly

diagnosed in each case (and that does require an ability to make differential diagnoses which many persons lack), it is still not advisable to complicate the picture, but rather to keep the genetic types separated into specific categories wherever that is possible. This problem is complicated and still not completely resolved. Geneticists and medical men should determine whether the "hemophilics" with unlike family histories can be separated into specific types on the basis of recognizable physiological differences.

Recognition of Biotypes

Human geneticists have been seeking methods which might help break down a single phenotype into various biotypes by sorting out detectable differences among persons who have been grouped together. At one time, epileptics were all considered to be alike and genetic interpretations were based upon that assumption. For a long time all feebleminded persons were placed in one category. Now investigators are beginning to use more refined methods of attack to differentiate the feebleminded group into specific types. One type can be separated from the others by tests for the presence of phenylpyruvic acid in the urine. Another type is possibly related to the incompatibility of the Rh blood types of the mother and fetus. Still other specific types will be separated out by continued research. Deaf-mutism apparently is divisible into more than one type on the basis of morphological differences. Yet one phenotype is common to all deaf-mutes; they lack the ability to hear and do not learn to talk. Attention should be called to those traits which are not always inherited alike in different families. This will remind the person interested in human genetics that he should find means of recognizing the various biotypes.

The assumption that a disease condition, or any set of similar symptoms must always be caused by the same gene in all families is based upon erroneous reasoning. Genetics has developed from a belief in unit character inheritance to the recognition that any character is the result of the interaction of many genes and also that any gene affects more than one character. Eye color in man is due to the interaction of several gene-pairs. In man the difference of one gene may cause one person to be normal and another to have mental deficiency, skeletal defects, and endocrine disturbances. In other families either of these defects may occur independently of the others. Sex-linked recessive vermilion and autosomal scarlet are identical in Drosophila in that both lack brown pigment in the eyes. Even though two non-allelic genes cause the same phenotype as we describe it, they do not always control the same physiological or developmental processes causing the described character. It is not necessary to assume that like traits which have different methods of inheritance in various families are caused by the same gene except that its position has been changed. Separating like individuals into their specific biotypes depends upon refined methods of analysis and upon experimentation. In human beings, this becomes especially difficult. Progress lies in the perfecting of such refinements, not in the return the author advocates to the simpler concepts of a generation ago.

The literature of human genetics trickles down a thousand small streams. To harness the flow a dam is needed. Gates' compendium, like the beaver's dam, is a remarkable accomplishment. In both cases one is forced to admire the patience and industry of the builder, and his keen eye for the tree, if not the forest. In their way such dams serve well enough, until the engineers arrive.

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A REVOLUTION IN SOVIET SCIENCE

C. D. DARLINGTON, F.R.S., D.Sc.

EDITOR'S NOTE: *The question of genetics in the Soviet Union has been the subject of several notes and articles in the* JOURNAL. *Certain aspects of this controversy still puzzle many people. Early this year a remarkable statement regarding certain aspects of the "Battle of the Darwinians and the Mendel-Morganians" appeared in the British Journal* Discovery (*Vol. 8, No. 2, February 1947*). *Since few in the United States will have an opportunity to read Dr. Darlington's discussion, permission to reprint this article was generously granted by the author and by the editor of* Discovery.

Earlier articles in the JOURNAL *have covered some of the ground of Dr. Darlington's article, but the repetitions are not serious. Nor do we feel that there is serious conflict with a pending article in appreciation of the life and work of Dr. Vavilov. Since Dr. Darlington wrote, new information has become available regarding the time and place and circumstances of Dr. Vavilov's death.*

SHORTLY after the Russian Revolution Lenin set to work to establish a system of scientific research. He was concerned that it should be of practical use but at the same time have sound theoretical foundations. With these ends in view, he set up the Lenin Academy of Agricultural Sciences with a young man, Nikolai Ivanovitch Vavilov, at the head of it.

Vavilov was a plant breeder educated partly in England, a pupil of William Bateson, and an energetic and original exponent of the new science of genetics. He began the organization of the Bureau of Plant Industry throughout the Union. He undertook expeditions to Afghanistan, Abyssinia and Central and South America, and his collections put all our ideas of the origins of cultivated plants in a new light. They also enabled Soviet breeders to work with the best possible materials in improving the crop plants needed for the new agricultural development of their country.

Vavilov's work came to be applauded and imitated throughout the world. In the Soviet Union itself, however, his success gave rise to some doubts. There had always been a feeling that genetics, with its rigorous determinism and its consequent assumption of ineradicable race differences, was not entirely in keeping with the general line of Soviet ideology. A view much more convenient politically had been expressed by old-fashioned botanists in Russia whose liberal views had led them to support the Revolution. Such men as Timiriazev and Michurin, although their opinions on heredity had been discredited in scientific circles abroad, gave a much more hopeful view of what the environment could do in changing heredity. And, as Marx had said, it is not enough to understand things. We have to change them. Men in Moscow were beginning, under Stalin, to drink deeper draughts of the Marxist doctrine and it proved to have, as it was intended to have, the most intoxicating properties.

Complaints about genetics were brought to a head in a very odd way. An obscure worker on an agricultural research station in the Ukraine claimed that, by special treatments, which he had presumably heard of from German sources, wheat could be made to grow farther north. His claim gave birth to propaganda, probably on a larger scale than he had expected, propaganda which preceded instead of following the success of the treatment by 'vernalisation.' The treatment in fact led to nothing except that it put its discoverer into a position in which he thought he might safely claim that he could change heredity. How he did so we shall see later. Such

was the beginning of the career of Academician Trofim Lysenko.

Lysenko alone would have been useless; but he chanced to fall in with a very active philosophical writer, of the name of I. I. Prezent. Together these two concocted a theory of genetics and this theory they presented in the correct Marxist terminology to a series of genetics conferences in Moscow and Leningrad. They presented it as an alternative to the western, bourgeois and clerical science of genetics as previously understood. The third of these conferences took place in 1939. It was completely successful. It was followed by the dismissal and death of Vavilov and his leading colleagues, and the appointment of Lysenko to his position as President of the Lenin Academy.

The meaning of these events has long been known to geneticists. The evidence, however, had not been readily accessible. The world is now indebted to Theodosius Dobzhansky, Professor of Zoology at Columbia University, one of the leading geneticists in the United States and a graduate of Kiev University, for the translation of a book by Lysenko which brings his theory up to date. This work was published in 1943 and circulated abroad in 1945, and again in 1946 bound with the author's collected articles in *Pravda,* under the title of *Heredity and its Variability.* It is a short book of some 25,000 words. It includes many repetitions and many obscurities but every word is worth reading by scientists outside Russia for these are the arguments, the ideas and the methods of the man appointed as the chief scientific director of Soviet Agriculture and a Vice-President of the supreme organ of a government which. we have been given to understand, attaches supreme importance to science.

Lysenko begins with these words:

> *Heredity as understood by followers of Mendel and Morgan.* In all reference and text-books on genetics, heredity is understood merely as reproduction of like by like. In my opinion, such a definition gives little basis for understanding the phenomenon of heredity. Men have always known that wheat grows from wheat seeds and millet from millet seeds, etc. . . .
>
> *Our conception of heredity.* Our definition of the phenomenon of heredity differs from that which has been accepted in genetics up to the present time. We understand heredity as *the property of a living body to require definite conditions for its life, its development and to react definitely to various conditions.* We understand the term heredity as referring to the nature of the living body. . . .
>
> The knowledge of conditions which are required by a living body, and of the reactions of that body to various conditions, amounts to a knowledge of its hereditary properties. Consequently *to find the conditions of the external environment which are demanded by a living body (an organism) for the development of certain characters or properties will amount to investigation of the nature, i.e., of the heredity, of those characters and properties.*

On this foundation (the italics are Lysenko's) the argument proceeds easily. Heredity is development. The environment can change development. Therefore the environment can change heredity.

Lysenko recognises the need for experimental evidence in support of this fundamental conclusion. "Comrade Solovey," he writes, "has, by means of autumn sowing, obtained a winter form from the spring barley 'Pallidum 032'." And again, "by vernalisation spring forms have been obtained from every standard winter wheat taken for experiment." Such statements by themselves, or from a trained geneticist, would be bound to arouse the greatest admiration. We have to remember, however, that Lysenko six years ago claimed to be able to revolutionise Russian wheats in three years by 'intravarietal hybridisa-

tion'*—a claim no longer mentioned in the present work. We also have to remember that Lysenko denies the existence of 'pure lines' and that the highest test of homogeneity† he applies is to take seeds of wheat 'from the same bag' (p. 47). In this light we can understand the 'forms' and 'varieties' he works with are in fact *mixtures* such as are found in primitive agriculture everywhere. Using such 'varieties' it naturally follows that "with bad agrotechnique one never can get good varieties from bad ones and in many cases even good cultivated varieties become bad in several generations."

When such 'forms' or 'varieties' are grown under extreme conditions (whether vernalised or not) they are at once exposed to selection and are rapidly changed in the well understood Darwinian way. The evidence as a whole shows that Lysenko is making use of the three classical precautions needed for the success of so-called Lamarckian experiments, experiments designed to prove the inheritance of environmental effects: namely beginning with a mixed stock, omitting to use proper controls, and repudiating statistical tests.

But it must not be supposed that any single or consistent fallacy runs through Lysenko's unfolding of his new science. It seems indeed unlikely that any fallacy known in the histories of science or of magic escapes Lysenko's grandiose mystification. But the prime evil lies not so much in fallacies (which are already well known in western countries) as in the method of argument from authority which, on a kindly interpretation, might have overflowed from the *Pravda* articles. "Our Soviet science, the Michurinist direction in science, gives a clear understanding of the way in which the nature of organisms may be changed." The naughty Mendelian-Morganians on the other hand "disagree also with the Darwinian theory of the development of plant and animal forms." And again "the best biologists, Burbank, Vilmorin and particularly Michurin, have paid much attention to the practical significance of plant organisms with destabilised heredity." Clericals and reactionaries like Mendel and Johannsen wasted their time presumably with undestabilised heredity. "Darwin repeatedly emphasised in his work the usefulness of crossing and the biological harmfulness of self-fertilisation as a law of nature." The author has not heard of any post-revolutionary work outside Russia and so does not know that this law of nature was recently destabilised, if not liquidated. "A correct classification of the facts of behaviour of hybridisation was given by K. A. Timiriazev." Timiriazev was a Russian physiologist, friendly to the revolution, who died in 1920. He never made a hybrid in his life. "Facts available in Soviet agrobiological science permit the formulation of a single operational theory of heredity which fulfils the requirements of K. A. Timiriazev . . ." What the facts are we are not told. What the theory may be we are left to guess. But the requirement of Timiriazev is fortunately no other than that of William of Occam.

Scholasticism goes of course with certain other signs of scientific decay. "An organism does not accept inferior food elements if superior ones are available," writes the physiological Academician. "The relative purposefulness, the adaptation, of the plant and animal world are brilliantly explained by the Darwinian doctrine of natural and artificial selection." Teleology unites with dialectical materialism, and natural selection is an equally good bedfellow with the inheritance of acquired characters. "If the surrounding milieu does not contain the conditions needed for the development of some characters and properties without which the organism cannot continue to exist, such characters and properties cannot easily remain recessive. They develop perforce, so to speak because otherwise the organism must cease to exist." Apparently it would not be dialectically sound for a well-bred or-

*That is, by making crosses between different plants of the same variety which should in wheat be identical.

†Uniformity of heredity.

THEIR MASTER'S VOICE
Figure 2

Central figure in this photograph is Academician Trofim Lysenko, here seen lecturing a group of visitors to the All-Union Agricultural Exhibition in Moscow.

ganism to cease to exist, so to speak. We could not help admiring the effrontery, were it not unconscious, with which the language of dialectical materialism is given a mystical content in a country where many men have been damned for philosophical heresy by Lysenko himself. "The normal internal contradiction of life, the life impulse, is created and from time to time renewed in the plant and animal worlds largely through crossing, fertilisation, sexual union of plant or animal forms at least slightly differing from each other."

A robust anthropomorphism is seen in such arguments as: "If plants and animals possessed infinite individual lives they would, speaking crudely, be in torture during all their lives," or at least, one must suppose, in their old age. However, even according to Lysenko's theory, sexual union is not accomplished without pain, for the dominant eats and digests its recessive allelomorph and belches it up later to give segregation. In spite of this 'assimilation' we learn that "The double heredity gives rise to a greater viability of the organisms, and to their greater adaptation to varying living conditions."

It is strange amongst all this fictional writing to find Lysenko devoting a page to a description of chromosome behaviour as it was understood before the Russian revolution—no doubt taken out of an early work by Morgan. In this he is hardly behind some of our universities but the reader is left wondering whether Lysenko believes this account has anything to do with heredity or not, in view of his extraordinary notions about statistics for, as everybody knows, statistics is a key to Morgan as well as to Mendel. He concludes:

> The Mendelians foist this 'pea law,' according to Michurin's happy expres-

sion, on the whole of living nature. But in reality it is basically wrong even for pea hybrids, including the factual material obtained by Mendel himself. The progenies of different hybrid plants varied even in Mendel's experiments much beyond the ratio 3:1. Thus, in the offspring of one plant there were 19 yellow and 20 green seeds, and of another plant—only a single green for 30 yellow ones.

Lysenko here seems to attach value, if not to sound statistics, at least to plausible numbers but elsewhere in the Proceedings of the Soviet Academy of Sciences he has demonstrated by irrefragable dogma that statistics have nothing to do with the case.

Many years of obedient and recently unanimous applause have given Lysenko an audacity which is bound to astonish the unprepared reader. It was in 1911 that Erwin Bauer, a Mendelian geneticist, showed that mutations in vegetatively propagated plants made them genetically mixed. A skin of the mutant propagated itself permanently over the original type of flesh, or vice versa. Chimaeras arise in this way in many, perhaps in most, old cultivated clones and it was a minor success of great practical interest when Asseyeva, one of Vavilov's school (in 1927) showed that many potato varieties were chimaeras. If adventitious buds were grown after the eyes had been cut out the new shoots would be free of skin and different from the typical variety. Lysenko quotes this fact. He quotes it four times—for him a moderate degree of repetition — as though it were his own discovery. He quotes it to prove four different things but first and foremost to show that:

> The assertion of Mendelian-Morganian geneticists that all the cells of an organism possess the same nature, the same heredity, does not stand criticism.

Asseyeva might have made an interesting comment. But Asseyeva is no longer there to reply.

One of the sacred authorities to whom Lysenko pays homage is that 'talented Russian genius,' Ivan Vladimirovich Michurin, who died in 1935 in the town now known as Michurinsk. Michurin was an importer of plants who, like the American, Burbank, accounted for many of his introductions as the result of his own remarkable success in hybridisation. His motto was: "We cannot wait for nature to grant us favours: it is our business to take them from her." Again like Burbank he took the favours and having done so he supported his claims to originality by a theory that extraordinary hereditary improvement could be made in plants by special tricks of ageing or grafting. This view finds no support in the 2,000 years of grafting experiments carried out before Michurin's birth chiefly outside Russia, and it is much easier to suppose that he got his best plants from Canada and the United States. Nor are our suspicious lessened by the statement that in certain crosses "it is preferable to use pollen of a young plant in its first year of flowering the nature of which has not yet become strong."

Michurin's theories, however superstitious to us, fit in very well with Lysenko's environmentalist propaganda and the general political trend in Soviet Russia. That is why today we learn that "Since Michurin's death his work has been continued not only by great scientists in selection and by various institutes but also by thousands of small laboratories set up in villages throughout the country. The huge army of Michurin experimenters form the vanguard of socialist agriculture."*

When we recollect that Michurin recommended the use of mixed pollen and that Lysenko finds no objection, on his digestion theory, to hybrids being purely maternal we cannot be surprised to learn that "fruit-bearing hybrids have

*Professor Paul N. Yakovlev, Stalin Prize Winner and Manager of the Michurin Nursery Gardens, in the Voks 7 8 U.S.S.R. Bull. Soc. for Cult. Relation with Foreign Countries, 1945, p. 51.

been produced in Michurinsk as hybrids of apple and pear trees, plum and peach, cherry and plum, red and black currants." Doubtless the production of these hybrids was part of the year's plan of research and the plan was fulfilled. How fortunate that the hybrids proved to be fertile!

It is of great interest to western observers to notice that, as the practical success of the Lysenko and Michurin schools becomes more demonstrably lacking, their claims to future success grow larger and their claims of Marxist purity and their denunciations of bourgeois and fascist science become more vociferous. We may learn a great deal in studying these claims from the examples of Titus Oates and the late Dr. Goebbels, or the South Sea Bubble and the Dreyfus case, historical figures and incidents normally outside the scientific curriculum. We may even, thinking of Russia, recall Rasputin. Those who have been privileged to hear Academician Lysenko rant and roar have compared him with Hitler. Certainly in manner and matter the literary works of the two masters bear some resemblance.

The Russian story is in fact as tragic as any of these names can suggest. It is one not to be remedied merely by the exposure of a few charlatans. The leading Russian geneticists (apart from those who have taken refuge outside Soviet-controlled countries) have been 'liquidated' in the course of this long political intrigue. These are no longer questions that can be argued about in Russia. They have been decided. All those who were prepared to argue have been put away.

Not only science but the people have paid. In the revolutionary campaign to produce a socialist agriculture whose scientific basis we now see the Russian people have made terrible sacrifices. *Delirant reges, plectunctur Achivi.*

We see indeed the official overthrow of truth and reason and of the men who stood by them in one branch of science. This overthrow is no less official than that in Hitler's Germany. It affects first and foremost the same branch of science, that concerned with heredity, race, and class. The reason is merely that this is the science of greatest political moment under the conditions of social stress in the world today. The implications of this situation reach beyond the scope of the present review. But men of science everywhere will do well to ponder them.

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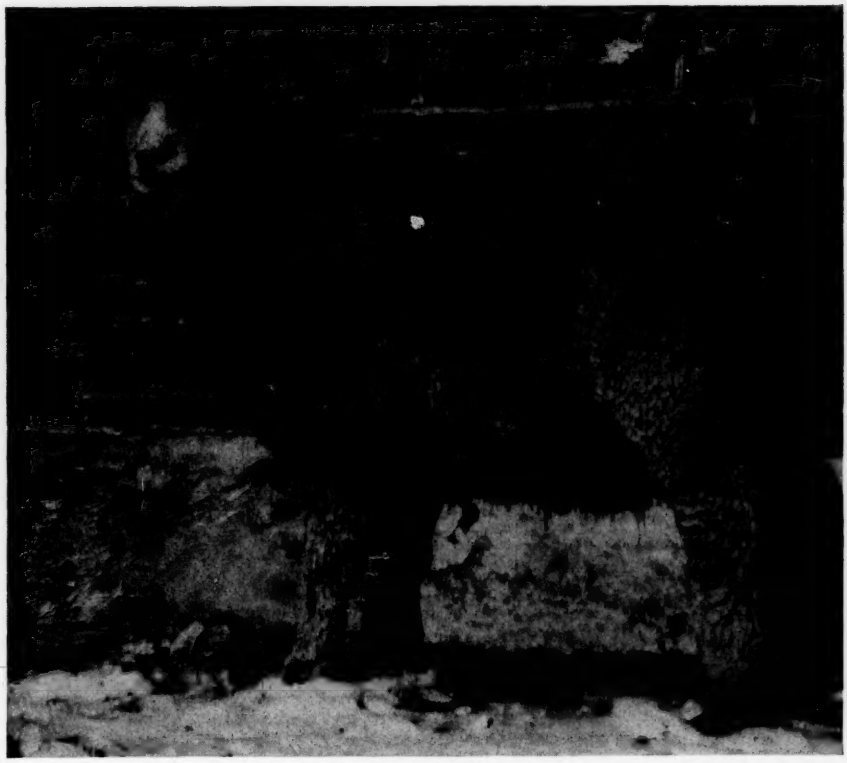
AN OVINE FREEMARTIN

ALASTAIR H. EWEN AND FRANCES A. HUMMASON*



HORNED EWE

Figure 3

This is a purebred Rambouillet ewe born twin to a ram. Starting life as a typical ewe, U. S. 79W developed full sized horns, which in this breed are normally found only in males. Anatomically the animal was intermediate in sex differentiation.

A FREEMARTIN is defined as an hermaphrodite female twin born with a perfect male. The term is usually applied to cattle, where the condition has been known for many hundreds of years. It has generally been regarded as a condition peculiar to bovines, since it has not been found in the other farm animals. However there have been reports of cases of intersexuality in horses, sheep and pigs, a few of which show some similarity to the case of the bovine freemartin.

Reported cases of hermaphroditism in

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the sheep are rare. Fraser-Roberts and Greenwood[2] in 1928 described a case of "a freemartin-like condition in the sheep" and Fincher and Williams[1] have reported cases of ovine intersexuality. An extensive discussion of the problem of "freemartinism" is found in the article by Swett, Matthews, and Graves.[3]

The case reported here occurred in the Rambouillet flock maintained by the University of Saskatchewan at Saskatoon, Canada.

The purebred lamb U. S. 79W (Figure 3) was born twinned with a normal male. The sire was Deer Lodge Farms Co. 4885, a normal horned Rambouillet ram, and the dam, U. S. 143U, a normal Rambouillet ewe. The lamb at first was ewe-like in appearance, with what seemed to be a normal vulva; it urinated as a ewe. Ultimately the lamb began to develop horns and it then became obvious that some abnormal development of the reproductive organs had occurred.

As a yearling the animal had the outward appearance of a normal Rambouillet ram with the strong horn growth typical of males of that breed but there was no external evidence of any penis. The vulva was normal but the clitoris was much enlarged and misshapen. The mammary glands were similar to those of a yearling ewe except that the right was smaller than normal.

In November 1945 the animal was slaughtered at the age of 1½ years. An examination of the reproductive organs revealed that one testis-like gland (the right) was situated immediately above the right mammary gland in a position which would have been normal had a scrotum been present. The vas deferens, if it can be so called, passed through the inguinal canal and opened into the vagina-like urogenital passage near the external meatus of the urethra. The left reproductive gland was situated in the normal position for an ovary, being attached to the posterior wall of the abdominal cavity behind the left kidney. A duct, which had the histological appearance of a vas deferens led from it to the urogenital passage.

The other organs appeared normal except that the thyroid gland was very much enlarged, being 78 mm. in length.

The tissues were dissected. Parts of the thyroid, suprarenal, urinary bladder, vagina, gonads, were taken, fixed in 10 per cent formalin, embedded in paraffin and stained with hematoxylin-eosin, iron-hematoxylin; also with methyl violet and Lugol for amyloid, silver nitrate for calcium and ammoniacalic silver for reticular tissue.

The histological examination revealed:

Thyroid: The follicles are greatly enlarged and filled with homogenous colloid. The lining epithelium is flattened in most of the follicles and there is very little interfollicular connective tissue. (Colloid goitre.)

Suprarenal: Normal.

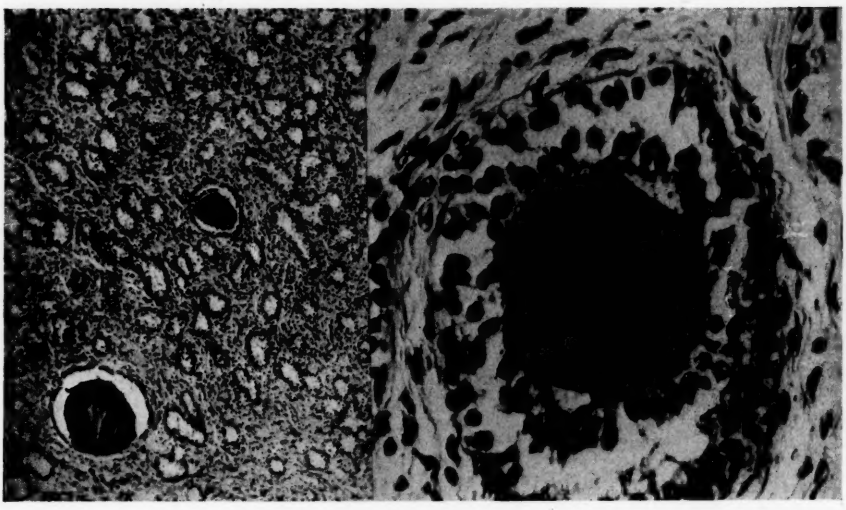
Urinary biadder: Normal.

Vagina: The narrow vagina-like structure has a high lining of stratified squamous epithelium with high connective tissue papillae. A formation which may be considered a clitoris, shows two different types of connective tissue: a) coarse fibers forming an alveolar-like framework and b) young, tender, basophilic connective tissue filling its meshes. This structure may well correspond to rudimentary or obliterated cavernous tissues.

The gonads: In both gonads a very similar structure is found, in spite of the fact that the left gonad had not descended and was smaller. Easily recognized on each side are the rete testis, the vas deferens, and the pampiniform plexus. The vasa deferentia are fairly wide and patent; the left is very thin-walled and its smooth muscle is greatly reduced.

The parenchyma of the gonads consists of tubules lined by simple epithelium. Its cells possess pale cytoplasm and a small round basally placed nucleus with scanty chromatin. The free borders of the cells are fringed and their filaments frequently formed irregular net-like structures. A thin, eosinophilic basement membrane can be recognized.

The course of the tubules is slightly curved (Figure 4). They are separated from each other by cellular connective tissue. In the small gonad the rete testis forms a boundary between the tubules described above and other tubules which differ from the above in that they are separated by connective tissue into alveolar-like formations and moreover many of them are undergoing atrophy and replacement by fibrous tissue. No interstitial cells of Leydig are found in the intertubular connective tissue.



SECTIONS OF GONADS

Figure 4

The gonads were in different locations, the right being situated as in the male, the left being undescended. Histologically both gonads were similar, and their structure was not normal for either sex. The dark concretion-like objects resemble dead ova, but structurally this interpretation is not satisfactory. The two views show different magnification of the sections of the tubules described in the text

A noteworthy feature in both gonads is the occurrence of concretion-like formations in some of the tubules (Figure 4). These formations are large, eosinophilic bodies with a circular stratification. They fill and extend some of the tubules. They appear, at least morphologically, identical with concretions which are found in the prostate of adult men and which increase in size with age. The differential stain with methyl violet or with Gram's iodine gives no reaction for amyloid, and Kossa's silver method is negative for calcium salts. The lining epithelium in those tubules which contains such a concretion is not single layered, but an additional laver of epithelial cells is adherent to the concretions. This cellular arrangement gives, at the first glance, the impression of a corona radiata of granulosa cells surrounding a dead ovum. This appearance does not correspond to facts because there are no other structures suggesting primordial ova in a stage before the transformation into concretions, and serial sections reveal that the structure, containing the concretions, is tubular and not follicular. The variable size of the concretions, in addition to its stratification, denotes a gradual increase by apposition. As to the origin of the concretions it may well be assumed that they are formed by a crystallization-like process, namely a loss of colloidal equilibrium in the mucus-like content of some tubules. This process may have been initiated by desquamated epithelial cells.

In spite of a thorough search no epididymis and no seminal vesicles were found; this is in contrast to other cases published. Likewise neither a prostate nor a uterus was present.

Summary

Ovine freemartins are of rare occurrence and therefore it was deemed worth

while to report such a case. Moreover, microscopic examination of some of the organs revealed various aberrations from the normal, the two main ones being a colloid goitre and gonads of different size. Microscopically the gonads showed undifferentiated tubules, but no interstitial cells of Leydig were found. Numerous tubules contained concretions similar to, or identical with, those found in the prostate of adult men. No structures resembling the epididymis, seminal vesicles, prostate gland or uterus were found.

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THE BARBARA BURKS MEMORIAL FUND

CONTRIBUTORS to the Barbara Burks Memorial Fund will wish to know that the decision has just been reached by the committee to apply the fund to completion of her research on identical twins reared apart. It has been discovered that her research material contains data on four pairs, three now adult, one adolescent. The cases are fairly complete and each will comprise a report comparable to the one already published by Dr. Burks in 1942.* It was felt by members of the committee that Barbara's investment of interest and energy in collecting the twin material had been very great, that such data are rare and difficult to assemble, and that they should not be lost.

Anne Roe, who generously reviewed and evaluated the material, has agreed to undertake the assignment of completing the study, and she will devote to it such time as she can capture from her other professional commitments. An advisory subcommittee consisting of Dr. R. S. Woodworth, Dr. Katherine Brehme, and Dr. Lewis Terman has agreed to confer with her when help is needed in the course of the work. The principal of the fund (about $1200) will be devoted to the project and its publication.

Delay in arriving at this decision has been due to a number of factors. It was agreed to postpone any action until after the war was over. Then a long search had to be conducted in order to find the material and further time was required to review and evaluate it and to consider other possible applications of the fund.

Other applications considered by the committee were: to use it as a loan fund to foreign psychologists working in this country; to finance a yearly award for the best work done in the field of genetic psychology; to provide a Barbara Burks Memorial Lecture each year at American Psychological Association meetings. The financial limitations of the fund seemed to make these applications impracticable. The committee hopes that contributors to the fund will feel satisfied with the appropriateness of the final decision. We report with great sadness the recent loss of one member of the committee, Kurt Lewin.

GORDON ALLPORT
KATHERINE BREHME
ROBERT COOK
THEODORE NEWCOMB
LEWIS M. TERMAN
RUTH S. TOLMAN, *Chairman*
ROBERT S. WOODWORTH

*"A Study of Identical Twins Reared Apart Under Different Types of Family Relationships" in *Studies in Personality* (volume in honor of Dr. Lewis M. Terman), edited by Q. McNemar and M. A. Merrill. Pp. 35-69. McGraw-Hill Book Company. New York 1942.

FUZZY LEAF IN COTTON AND ITS ASSOCIATION WITH SHORT LINT*

D. M. SIMPSON†

THE pilosity of the cotton plant in relation to insect control has been the subject of a number of investigations. Parnell[3], Peat[4], and others in South Africa found positive correlation between density and length of hairs on the cotton plant and its resistance to jassid injury. Dunnam[1] studied pilosity in relation to adherence of calcium arsenate in boll weevil-control experiments and found that pilose cotton retained more arsenic pentoxide. Dunnam and Clark[2] studied pilose and glabrous cottons in relation to the cotton aphid and found that the aphid population increased in direct proportion to the number of hairs on the lower leaf surface. More recently, pilosity has assumed importance in relation to trash content and cleaning of mechanically picked cotton.

No studies on the inheritance of pilosity have come to the attention of the author. The recent isolation of a major gene affecting this character and its relation to fiber properties is reported here.

Materials and Methods

In 1936, a cross was made between Trice and Tidewater, two varieties of upland cotton (*Gossypium hirsutum*). Several of the first-generation hybrids were backcrossed to Trice in 1937 and continued as selfed progenies in 1938 and 1939. One of the F_3 progenies grown in 1940 contained a single plant strikingly different from its sibs. It had very dense pubescence, which gave the leaves a whitish cast and the feel of velvet. The pubescence was present on all the external surfaces of the plant, including stems, bracts, bolls, and even the petals of the flowers. (On a later selfed generation of this plant, hair counts showed an average of 12.0 hairs per square millimeter on the upper surface of the leaves and 18.8 on the lower. The counts were made on the fourth expanded leaf from the top of the plant; midribs and veins were excluded from the area on which counts were made.) The plant was of normal size and vigor and compared favorably with its neighbors in productivity. The fiber, however, was very short, averaging about 28 thirty-seconds of an inch in length as compared with 36 thirty-seconds for the progeny group from which it came. This aberrant plant was observed early in the season, and self-pollinated seeds were obtained from it. In the subsequent inbred generations from this plant there has been no observable segregation in the leaf, boll, or fiber characters. This pilose line, designated T611, is apparently a mutant carrying a gene for pubescence not previously described in upland cotton.

To test the agronomic possibilities of this extreme pubescent character, crosses were made between T611 (pilose) and two local inbred strains of upland cotton that have nearly glabrous leaves. These strains were Acala 5675 and Stoneville 17. From the two crosses, 23 F_1 plants were grown in 1943, 100 F_2 plants in 1944 and 469 F_3 plants in 1945.

Inheritance of Pilosity

In the first and subsequent generations of the crosses between T611 and the strains with nearly glabrous leaves, the plants were classified on the basis of leaf hairiness. The differentiation of three classes was relatively easy, the full-

*Contribution from the Division of Cotton and Other Fiber Crops and Diseases, Bureau of Plant Industry, Soils, and Agricultural Engineering, in cooperation with the Tennessee Agricultural Experiment Station.

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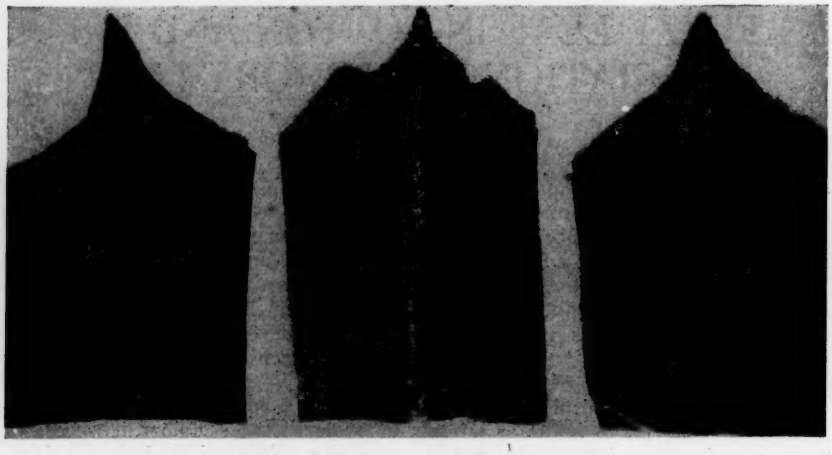

SEGREGATION IN LEAF-HAIRINESS
Figure 5

Leaves from three plants in the progeny of a plant with leaves intermediate in hairiness. The leaves are classified as glabrous, intermediate, and pilose (at right). The glabrous and pilose forms are true breeding, intermediate form representing the heterozygote.

pilose plants being considerably more hairy than the intermediates (Figure 5). Differentiation between pilose and intermediate-pilose plants was aided by the fact that the bolls of the pilose plants were decidedly hairy, while those of the intermediate class were nearly glabrous. (Figure 6.) The three classes of plants could be distinguished readily by the feel of the leaves and the unopened bolls.

The frequency of pilose, intermediate, and smooth-leaved plants in the parent populations and in the first, second, and third generations of the hybrids is shown in Table I. All F_1 plants were intermediate, with incomplete dominance of pilosity. The segregation in the F_2 populations closely approximated a 1:2:1 ratio, indicating that the pilose character is controlled by a single gene. This was confirmed by the segregation of the F_3 populations from the three F_2 leaf-class phenotypes. All progenies from pilose F_2 parents were pilose, all progenies from smooth F_2 parents were smooth, and progenies from intermediate F_2 parents segregated for pilosity in frequencies corresponding satisfactorily to the expected 1:2:1 ratio of pilose, intermediate, and smooth.

Association of Lint Length and Pilosity

Lint length was determined from parent and hybrid plants when adequate samples were available. The pilose parent, T611, had very short fiber, averaging 27.7 thirty-seconds of an inch in length in three years of test; Acala 5675 has somewhat longer fiber, averaging 36.4 thirty-seconds of an inch during three years; and Stoneville 17 averaged 33.5 thirty-seconds of an inch in two years of test.

Length of lint by class intervals for the first, second, and third generations of the crosses is given in Table II, together with the leaf classes of the parent plants and their progeny. The lint length of the first-generation hybrids was intermediate between that of the parent strains, but more nearly approached the length of the longer parent.

In F_2, the fiber from the pilose-leaved plants was approximately the same length as from the pilose parent and

BOLL-TYPES ASSOCIATED WITH HAIRINESS
Figure 6

Bolls from the three plants shown in Figure 5. While the bolls of the intermediate type appear to be almost as smooth as those of the glabrous plant, there is enough hair on them so that the three classes can be distinguished by touch. So consistently is the pilose condition associated with extremely short lint, that the pleiotropic effect of a single gene is suggested.

in no case approached the mean length of the longer parent. The smooth-leaved F_2 plants had fiber of approximately the same length as the smooth-leaved parent, and the intermediate-leaved F_2 plants had fiber of intermediate length. The segregation for lint length in the F_3 populations followed the mode of inheritance indicated in the F_2. All progenies from pilose F_2 parent plants were hairy and had short lint; all pro-

TABLE I. Frequency of pilose (P.), intermediate-pilose (I.), and smooth-leaved plants (S.) in the parent population and in the first, second, and third generations of the hybrid.

Parent Types	Leaf class Parent	Progeny P.	Progeny I.	Progeny S.	Tot.
F_1 (1943)					
T611	P.	13			13
Acala 5675	S.			13	13
T611 × Ac.5675	P. × S.		12		12
Stoneville 17	S.			15	15
T611 × St. 17	P. × S.		11		11
F_2 (1944)					
T611 × Ac.5675	I.	12	26	12	50
T611 × St. 17	I.	16	21	13	50
F_3 (1945)					
T611 × Ac.5675	P.	54			54
	I.	28	74	33	135
	S.			40	40
T611 × St. 17	P.	86			86
	I.	24	50	31	105
	S.			49	49

TABLE II. Association of short lint and pilose leaf in F_2 and F_3 segregates from crosses of pilose (P.) × smooth-leaved (S.) cottons.

Parents	Leaf class Par.	Leaf class Pro.	Mean lint length	Length of lint by class intervals (1/32") 23-25	26-28	29-31	32-34	35-37	38-41	Tot.
F_1 (1943)										
T611	P.	P.	28.2		2	2				4
Ac. 5675	S.	S.	36.5					11	2	13
T611 × Ac.	P.×S.	I.	34.7				2	3		5
St. 17	S.	S.	34.5					1		1
T611 × St.17	P.×S.	I.	34.2				3	2		5
F_2 (1944)										
T611	P.	P.	26.3	1	8					9
Ac. 5675	S.	S.	36.3				1	4	2	7
T611 × Ac.	I.	P.	25.9	2	9	1				12
		I.	31.4		1	12	13			26
		S.	35.1			1	2	8	1	12
St.17	S.	S.	32.4			2	4	1		7
T611 × St.17	I.	P.	26.2	5	10	1				16
		I.	31.0		1	13	7			21
		S.	33.9			2	5	5	1	13
F_3 (1945)										
T611	P.	P.	28.5		2	4				6
Ac. 5675	S.	S.	36.4					19	7	26
T611 × Ac.	P.	P.	28.1	3	13	14	1			31
	I.	P.	27.3	6	14	7	1			28
		I.	32.0			33	35	4		72
		S.	35.2			1	9	19	1	31
	S.	S.	36.2				1	20	5	26
St.17	S.			Not grown						
T611 × St.17	P.	P.	28.2	2	31	20	2			55
	I.	P.	28.6	3	6	11	2			22
		I.	33.4			7	23	10	2	42
		S.	34.7		1	1	11	9	3	25
	S.	S.	34.8			1	19	19	4	43

genies from smooth-leaved F_2 parent plants had smooth leaves and long lint; and progenies from intermediate-pilose F_2 parent plants segregated in a ratio approximating 1:2:1 both for leaf class and for length of lint. In all cases the mean length for each leaf-class phenotype group indicated close association of the pilose condition and short lint. There was some overlapping of length in the pilose and intermediate-pilose classes and in the intermediate-pilose and smooth-leaved classes, but the distributions around the respective means were no more than ordinarily occur from physiological causes in homogeneous populations.

The very close association between pilose leaf and short lint suggests strongly the pleiotropic effect of a single gene. The evidence at hand does not preclude the possibility of close linkage of the gene for pilosity with another gene having a major influence on lint length. As no such gene is known in the parent material, however, such a hypothesis would require its origin by mutation simultaneously with that of the pilose mutant. The simpler explanation seems the more probable.

In the segregating populations, there was no particular relationship between pilosity and seed weight, lint percentage, lint index, and lint strength. In general, the longer fibers were also finer, but this is a common physiological relationship, and concurrence between pilose leaf and coarse fiber is not necessarily demonstrated.

Summary

An extremely pilose upland cotton, apparently a mutant, is described. The simple mendelian inheritance of the pilose condition indicates control by a single gene allelic to the glabrous condition, with the heterozygote definitely intermediate in pilosity. The pilose condition and short lint have been so universally associated in the plants studied as to indicate that both conditions are the pleiotropic effects of a single gene.

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The Ancient Benefactors

SOMETIMES people do something without being aware of the eternal significance of their action. The giving of a cup of cold water to a thirsty soul may be of eternal significance. Little did farmers of ancient times appreciate the significance of selecting seeds of some wild grass that became wheat. The strain of wheat called Rival which you now grow more generally in South Dakota has an interesting history. This strain is the result of selection and plant breeding of scientists of today, but it began with a wheat plant that was developed from a wild grass by unknown and unsung primitive agronomists in the ancient past. This wheat had been passed on from generation to generation to our modern plant breeders who have selected a strain suited to special conditions in South Dakota. Thus far, all the science in plant breeding of our learned agronomists, subsidized by public funds, has not equaled the achievements in developing the wheat plant by unknown and unsung plant breeders of ancient times. It is probable that these our benefactors were women, for at first the women tended the fields in primitive societies. And it was their keenness in selecting seed of the more productive grass plants for the next crop that in time yielded the wheat plant that became so important in supplying food, and in furnishing the stock out of which your Rival variety of wheat has come.

Little did those people of ancient times imagine the significance of their contribution to the food supply of the world. We should honor these people of the past to whom we owe so much. Why don't we give more attention to such contributors to the welfare of civilization than to those who devise means of destroying our civilization in the twinkling of an eye?

So I say when we are occupied doing something that assures the welfare of our families, ourselves today and tomorrow, we may at the same time be doing something that has significance for the future and for the world.

W. C. Lowdermilk

COLCHICINE TREATMENTS FOR USE WITH TOMATOES

G. W. BOHN*

THE doubling of chromosomes by colchicine treatment offers new possibilities for the use of hybridization to shift valuable characters from wild relatives to domesticated species of plants. The author is engaged in a tomato breeding project whose aim is to combine in the commercial tomato, *Lycopersicon esculentum* Mill., the disease resistance and the high vitamin content of the wild tomato, *L. peruvanium* (L.) Mill.[2] Tetraploid plants of the two species were desired for use in cross pollination experiments, and our experience with producing such plants in quantity with colchicine are reported herewith.†

To increase the scope of the breeding experiments as well as to obtain the desired quantity of data it was considered desirable to use tetraploids derived from several seedlings rather than to use identical plants derived from single seedlings by the propagation of cuttings. It was considered likely that the desired numbers of tetraploid seedlings could be obtained from colchicine-treated seeds or seedlings.‡

Because tomato seeds are relatively tolerant of water and rather simple to treat in large numbers of individuals, seed treatments were selected for study. Particular attention was directed toward the maintenance of conditions considered to be most conducive to rapid growth of the tomato seeds and seedlings as recommended by Straub[10]. A preliminary experiment yielded several tetraploids of *L. peruvianum* but very few of *L. esculentum*. When treatments were adjusted a similar experiment yielded abundant tetraploids of both species and permitted statistical treatment of the results.

Materials and Methods

Seeds about six months old that had been cleaned by fermentation (five days at ± 60° F.), washed with water, dried, and stored at ± 70° F. in sealed envelopes were selected for use. These were counted into lots of 60 and placed in individual 140 ml. widemouth bottles 1¾ inches in diameter. The seeds were pre-soaked for one hour in 0.4 per cent colchicine solution; the excess was then poured off and replaced with four ml. of the same solution. This quantity was sufficient to cover the lower half of each seed and it was believed that this quantity would not interfere seriously with oxygen absorption by the seeds. The concentration of colchicine was maintained relatively constant by pouring off the solution once each day and making the volume equal to four ml. by adding distilled water to replace that lost by evaporation. The seeds were washed with water on the third day, pre-soaked in fresh colchicine solution, and measured quantities of fresh solution again added. The bottles of seeds were kept in moist seed germinators at 30° C. during the day (nine hours) and 20° C. during the night. Seeds were removed from the germinator at intervals of 48, 72, 96, and 120 hours, washed for an hour in three changes of tap water, spot-planted singly in 60-spot flats of steamed soil, one vial to each flat, and placed in the greenhouse (75° to 85° F. day temperature and 58° to 63° F. night temperature). Seeds treated in like manner with tap water for equal periods served

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†The opportunity is taken here to thank Paul Hepler, former Junior Scientific Aide, for able assistance with all phases of the experimental work.

‡Although several authors[5,6,7,8,9] have successfully applied colchicine to seeds and/or shoots of tomatoes to double the chromosome complements, the numbers of individuals used were small and the advantage of one treatment over another was not demonstrated. For a general discussion of colchicine treatments see Blakeslee and Avery[1], and Dermen's excellent review[3].

as checks. Four replications of each of three tomato varieties, *L. esculentum* varieties Danmark and Waltham Forcing, and *L. peruvianum* var. *dentatum* PI 128645*, were randomized in a split block arrangement primarily for the five treatments and secondarily for the three tomato varieties within treatments. The randomization was applied to the seeds in the germinators and was maintained in the arrangement of flats in the greenhouse.

Data were recorded on emergence, vigor of plants, and numbers of slow-growing plants with thick, swollen cotyledons. Hawkes[4] has demonstrated that the enlargement of cotyledons results from a secondary effect of colchicine and does not necessarily indicate polyploidy. However, it does indicate that the plant has absorbed colchicine in sufficient quantities to be affected, and observations on the mature leaves of older plants produced from such seedlings in this study indicated that all were polyploid. A few failed to produce shoots. An *L. peruvianum* plant produced one tetraploid shoot and one diploid shoot. A few other plants contained mixtures of diploid and tetraploid tissues or of tetraploid and octoploid tissues. Some flowers of such plants were found to contain both diploid and tetraploid pollen-mother-cells within the same anther. Three such plants ultimately yielded entirely *tetraploid* shoots that were readily recognized by fertility and by morphological characters of their leaves, stems, and fruits. These observations were verified by chromosome counts. No diploid shoots were observed on such plants. A few plants of *L. esculentum* were also apparently myxoploid, octoploid, or 16-ploid. No myxoploids with diploid and tetraploid pollen-mother-cells in the same anther were noted in *L. esculentum*. This difference may indicate differences in compatibility of diploid and tetraploid tissues in the two species or it may result from different effects of tetraploidy on growth rates. Tetraploid *L. peruvianum* was approximately equal in rate of growth to its diploid; tetraploid *L. esculentum* grew more slowly than its diploid. One apparently octoploid plant of *L. esculentum* was grown for several months. It grew very slowly and produced much-thickened and malformed plant parts including stems, leaves, and flowers. It failed to set fruits from self-pollinated flowers.

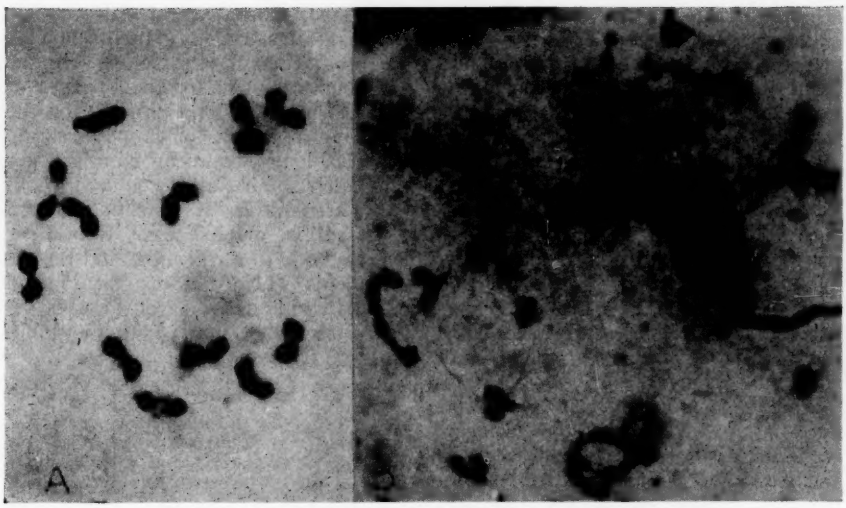
Although a few such aberrant plants were obtained in both species, most of the affected seedlings produced only tetraploid shoots that were vigorous, self-fertile, and readily recognized by morphological characters. Thirty-five plants of each variety selected as apparent tetraploids for use in the breeding work were found by chromosome counts of meiotic figures in aceto-carmine smears to be tetraploid; i.e. they contained 48 chromosomes in pollen-mother-cells (Figure 7) and 24 in microspores. However, the three myxoploid plants of *L. peruvianum* that produced fully tetraploid shoots were included. Similar studies of meiosis in non-treated plants demonstrated that these were diploids; i.e. they contained 24 chromosomes in pollen-mother-cells and 12 chromosomes in microspores.

Results and Discussion

An analysis of variance of emergence data indicated that the only significant variation in emergence among the treated and check seeds was caused by varieties; i.e. none of the treatments reduced the numbers of seedlings obtained. The mean percentages of emerged seedlings obtained from the different varieties were: Danmark, 87.5 per cent; Waltham Forcing, 95.8 per cent; *L. peruvianum*, 90.2 per cent.

Data on seedling vigor were recorded by assigning arbitrary numerical grades to the different sizes of seedlings 30 days after the seeds were started to soak. The most vigorous seedlings were rated 5; average seedlings, 3; and the weakest seedlings, 1; ratings 4 and 2

*Hereinafter referred to as *L. Peruvianum*. The P.I. number is the number assigned to this importation by the U. S. Bureau of Plant Industry, Soils, and Agricultural Engineering, Division of Plant Exploration and Introduction.



DIPLOID AND TETRAPLOID WILD TOMATO NUCLEI

Figure 7

Late meiotic prophase of nontreated *Lycopersicon peruvianum* at left showing 24 chromosomes arranged in pairs (12 bivalents). Late meiotic prophase of colchicine-treated *L. peruvianum* at right showing 47 of the 48 chromosomes arranged mostly in pairs (17 bivalents), but with one ring and two chains of four (three tetravalents). A member of one of the bivalent pairs lies underneath a group by the nucleolus. × 1500.

being intermediate. To avoid confounding other effects of the treatments on seedling growth with that resulting from polyploidy, only the apparently diploid plants were included in these ratings. A numerical grade was assigned to each flat as a unit.

An analysis of variance of these data indicated that varieties and treatments were significant sources of variation in vigor. The mean ratings of vigor for varieties were: Danmark, 3.5; Waltham Forcing, 3.8; *L. peruvianum*, 3.0. The mean ratings for treatments were: check, 4.0; 48 hrs., 4.2; 72 hrs., 3.3; 96 hrs., 3.0; 120 hrs., 2.5. Soaking the seeds for 48 hours in 0.4 per cent colchicine had no significant effect on seedling vigor but the longer treatments had a progressively greater detrimental effect. This effect probably resulted from an inhibition of growth of the young plants while the seeds were in the solutions and immediately following their transfer to soil. The seeds soaked in water were more advanced in development at 72 hours or longer than were those soaked in the colchicine solution for equal periods. This inhibitory effect was also indicated by the slower emergence from the soil of seedlings from seeds that received the longer treatments. Although the total numbers of seedlings were not affected, their rates of growth were.

There were no seedlings with swollen cotyledons among those from seeds soaked in tap water. There were very few such plants among those from seeds soaked in 0.4 per cent colchicine for 48 hours. The longer treatments with colchicine yielded progressively larger numbers of affected seedlings. An analy-

TABLE I. The mean percentages* of plants with swollen cotyledons obtained from four 60-seed replications treated with 0.4 per cent colchicine or water.

Treatment	Danmark	Waltham	*L. peruvianum*	All varieties
Water Colch.	0.0	0.0	0.0	0.0
48 hours	1.4	4.3	4.1	3.3
72 hours	9.4	9.4	17.0	11.9
96 hours	11.2	12.7	33.8	19.2
120 hours	18.9	28.2	51.2	32.6
All colchicine treatments	10.3	13.5	26.5	16.7

*Percentages based on numbers of seedlings.

sis of variance of the data from the 72-hour, 96-hour, and 120-hour treatments showed that both varieties and treatments were significant sources of variation. The mean percentages of affected seedlings from the three varieties for all treatments are shown in Table I. Because the colchicine treatments did not cause significant variation in emergence and differences among varieties did, truer figures for the effects of the treatments could be obtained by correcting the figures for the differences in emergence. The percentages in Table I are therefore based on the numbers of emerged seedlings.

These data indicate that *L. peruvianum* was more sensitive to the colchicine seed-soak treatments used than were the Danmark and Waltham Forcing varieties of *L. esculentum* and that Waltham Forcing was more sensitive than Danmark. These different sensitivities of the varieties probably resulted in part from the differences in the rate of initial growth.

Although the affected seedlings included some plants that grew very slowly or failed to produce shoots, probably as the result of larger chromosome numbers, most of these affected seedlings yielded total tetraploids that grew vigorously and were readily recognized by their gross morphological characters.

The most satisfactory treatments for the production of tetraploids were the 120-hour treatment for the varieties of *L. esculentum* and the 96-hour or the 120-hour treatment for *L. peruvianum*. The 120-hour treatment with 0.4 per cent colchicine has also been used to obtain tetraploid *L. pimpinellifolium* and *L. hirsutum*, amphidiploids from their hybrids with *L. esculentum*, and sesquitetraploids (6N)* from sesquidiploid (3N) hybrids derived from crosses between tetraploid *L. esculentum* and diploid *L. peruvianum*.

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*Sesquitetraploid is used here in the same sense as sesquidiploid[2]. It indicates a plant that contains four sets of chromosomes from one species plus two sets from a different species.

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